Reviewer’s report

Title: Incidence and Risk Factors of Retinopathy of Prematurity in Iran: A Systematic Review and Meta-Analysis

Version: 1 Date: 20 Nov 2017

Reviewer: Eduardo Villamor-Martinez

Reviewer’s report:

I commend the authors for a comprehensive meta-analysis of the incidence of ROP in Iran. Overall, their methodology and conclusions are sound, although there are several points where the methodology could be better, and where I would like some clarifications. My comments on the content are below. The written English could be revised and improved, and my comments on language are included after the comments on content.

In the introduction (p3, lines 25-38), the authors discuss the first and second ROP epidemic. No references are provided for these statements.

In the methods section, (p4, lines 17-19, 1.1 Study Protocol) the authors mention conducting the study according to PRISMA, but no filled-in PRISMA checklist is included as part of the supplementary material.

1.2 Search strategy (p4, line 40). The search strategy included in Appendix seems very broad, compared to the amount of results the authors report in the search diagram, 420. When I try to replicate theirs search, inputting search #1 into Pubmed, "Retinopathy of Prematurity [MeSH Terms]", I get 5211 results. If the authors could clarify their search strategy, I would appreciate it.

(p4, line 51, 1.3. Inclusion and exclusion criteria) "1. Non-random sample size for estimating the incidence". I am unsure what is meant by non-random sample size, or how it was used as an exclusion criterion.

1.5 Selection of studies (p5, lines 10-21): This section could benefit from more explicit assignation of responsibilities, such as through the use of author initials in the text. The authors mention that they blinded the researchers, but it is unclear who did the blinding and who was blinded. Presumably at least one author was not blinded since they carried out the blinding.

1.6 Quality of studies (p5, lines 36-37): "Finally, the studies that obtained the lowest score (15.5), were selected for the meta-analysis stage". I think they authors mean to stay that the lowest scoring studies were excluded from meta-analysis, with a score of 15.5 or lower.

Moreover, I would be interested in the number of studies that had a clear definition of ROP, and whether the authors contacted any primary
1.8 Statistical analysis (p5, line 59, p6, lines 4-5): The authors mention that they used a fixed or random effects model based on whether the meta-analysis showed low or high heterogeneity. It is good practice to determine the model to be used a priori, before having looked at the results or heterogeneity. Moreover, the test of heterogeneity suffers from low sensitivity, and some authors argue that in meta-analysis the random effects model should always be used (see: Borenstein M, Hedges LV, Higgins JPT, and Rothstein HR. Introduction to Meta-Analysis: Wiley, 2011.).

The authors also do not specify which model they chose to use in the results section.

2.9 Meta-regression, Figure 6 (p7, lines 35-37): The presentation of this result could be improved. Although the p-value for the meta-regression is provided, visual inspection of the plot shows heterogeneity, no coefficient and 95% confidence interval are given, and Figure 6 has a y-axis without labelled units. It is unclear to me whether this is a real effect or noise, and the authors do not mention it in the discussion. They do mention it in the abstract, but I believe this result would need to be presented less strongly.

3. Discussion (p9, line 30) and Table 4: the methodology for selecting these comparison studies is unclear to me. They seem to be regional reports, but this is not clarified, and it does not seem to be comprehensive.

In table 1, the authors provide "prevalence (%)" per included study. I believe they would better use incidence, since it is the indicator they describe as extracting in their methodology, and the one I believe to more appropriate in this case.

Figure 3: This figure does not add much to the report, since there are many included studies and I presume the authors used a random effects model, so the removal of any single study will have a minimal effect on the summary effect. Reporting in text in the results seems sufficient to me.

Figure 2 and Figure 5. In figure 2 a much larger number of studies is included, compared to Figure 5A to D. I assume this is because more studies reported on All ROP, and fewer reported subcategories of ROP, but I would appreciate if this was clarified in the results. Moreover, when reporting the incidence per stage (2.8 The incidence of ROP base on stage, p7, lines 25-31), it would be good to include the number of studies that reported each stage alongside the incidence.

The written English could to be improved significantly. These are some of the errors I found, or corrections I would make:

p2, line 8: "the incidence of ROP in Iran is different". Different compared to what?

p2, line 13: "until May 2017". I assume this refers to the cut-off date of the search strategy, not to the review article as a whole, so it does not make sense in this sentence.

p2, line 34: "related". Change to "associated with"

p2, line 35: "was significant increasing". Change to "significantly increased with year of study" or similar.
"significantly preventable". Change to preventable.

"can be". Change to "are".

"samples is more". Change to "the sample is larger".

"samples is more". Change to "the sample is larger".

"samples is more". Change to "the sample is larger".

"samples is more". Change to "the sample is larger".

Prepared Reporting Items for Systematic Reviews and Meta-Analyses.

"Sample size". Remove "size".

"browsers". Change to researchers or authors.

"includes". Change to "included".

"sub-group analysis of ROP was carried out". Change to plural.

"a sample size of". Change to "a total sample size of".

"much more". Change to "higher".

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

Quality of written English
Please indicate the quality of language in the manuscript:
Declaration of competing interests
Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare I have no competing interests.

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal